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Pacific islands, and from Iceland. I am, therefore, inclined to believe it is a volcanic product."

My analyses approximated Professor Huston's very closely, though I found mica scales, a trace of sulphur, in one analysis, with nearly one per cent vegetable matter in my first sample analyzed (this one procured from Mr. Teeter).

In precipitating the matter by melting the snow, the heavier portions fell to the bottom, and unless care was used the larger portion of the vegetable matter would be lost through being poured off. I found the coarser grains of silica (white sand) to be water-worn and scratched. Lime particles adhered to the sand-grains, just as one finds on the shores of lakes or rivers. Of the vegetable matter, I found the seed of a wild pea (*Lathyrus ochroleucus*), growing abundantly all over the Northwest. This seed, to make sure of no mistake I planted and grew the vine to maturity. Among wood-fibres identified were poplar (*Populus tremuloides*), pine (*Pinus strobus*), and casex (*Casex tenella*).

Now as to the source of this matter. It is plainly terrestrial; and, as the whole area traversed by the winds that carried it were covered with snow at the time, it is evident that it could not have been raised east of Lake Michigan. Its constituent elements preclude all possibility of its being meteoric or volcanic matter.

The fact that the sample analyzed by Professor Huston closely approximates certain volcanic samples can easily be accounted for on the ground that the precipitated mass was not homogeneous, and what was sent him could only have represented a portion of the mass, as another portion of it, sent to me by Mr. Teeter, out of the same lot, contained one per cent of vegetable matter, mica scales, and three small copper pyrites (yielding sulphur on ignition).

Every element of this matter is met with in abundance throughout all portions of the Northwest, and nowhere else do we find all of them on the surface. I conclude that this volume of matter must have been raised somewhere northwest, being carried south-eastward until it encountered the area of high-pressure that extended north of Chicago, and deflected in its course and fell within the area mentioned above.

Can anyone throw more light on the subject?

A. N. SOMERS.

La Porte, Ind., March 21.

The Aurora.

I HAD thought that no matter what Professor Ashe might say in regard to my note printed in *Science* for April 28, I would refrain from further comment. Inasmuch, however, as he in effect demands that something further be said, as appears in the closing paragraph of his note printed in *Science* for May 19, p. 277, I presume that I have no option but to comply. The point to which he asks special attention is as to the element of "chance" affecting the conclusions at which I have arrived respecting the location upon the sun of the seat of the activities originating the aurora in any given instance. The manner in which he puts this inquiry, as well as the general drift of his criticism, shows that I have failed to make myself understood in spite of very persistent efforts in the various notes and papers which he mentions, and which certainly, therefore, must have been taken into consideration in the comments made in the letter above mentioned. This being the case it will be necessary to begin at the beginning and state the heads of the argument by which my conclusions have been reached, so that if there is any flaw in the reasoning its precise location may appear and so that it may be explained also once again what are the precise conclusions for which I have been contending. The substance of the argument, stated in a few propositions as briefly as possible, is as follows: The agreement between the curves, representing the frequency of auroras, magnetic storms and sunspots is exact, and the nature of these phenomena is such that there can be no doubt whatever that the aurora owes its origin to a special form of solar activity. This proposition can be controverted successfully only by denying that there is such agreement as is claimed of the curves mentioned, or by advancing some alternative explanation of their connection with each other which will leave solar activities out of the question. Until this is done, this proposition

must stand, the evidence in its favor being adequate and there being no evidence pointing in a different direction. The solar origin of the aurora being thus established, its manifest periodicity at intervals of $27\frac{1}{4}$ days must be explained in accordance with its solar origin. If this can be done, the proof of such origin will incidentally be greatly strengthened. Now this period is totally indistinguishable from that of a synodic revolution of the sun—giving every evidence of being absolutely the same. This being the case we are able to formulate proposition number two to the effect that there is a periodicity of the aurora corresponding to the time of the rotation of the sun as seen from the earth. Here again the evidence is adequate and there is no evidence pointing to any other possible explanation. These two propositions being established there follows another, from which there is in the very nature of the case no possibility whatever of escape, and which is to the effect that whatever it is upon the sun which is capable of producing the aurora, it has this power during a very limited portion only of each revolution, which portion always remains the same during succeeding revolutions relative to the position of the earth in its orbit, otherwise the periodicity described could not exist. It remains only to identify the point whence the auroral effect proceeds. The period of auroral recurrence and that required for the completion of a synodic revolution of the sun as determined from the average rate of motion of spots being identical, there is no other way than to study the appearance of the sun at times of auroral recurrence in order to learn whether such recurrence is attended by any characteristic features. Thus it is found that no matter what appears elsewhere on the sun at such times there are always at the eastern limb areas on which spots are frequent and persistent. Thus the evidence is adequate that there must be something in that location in such cases which is responsible both for the sunspots and the aurora, and there is no evidence pointing in any different direction. On the contrary, the manner in which magnetic storms begin and the exactness of the periodicity manifest in their times of beginning are such as are totally inconsistent with any other explanation than that the originating impulse is brought to bear by coming into range suddenly around the sun's limb. But be this as it may, such behavior corresponds precisely with what is known in regard to the operation of electro-magnetic induction in which very precise arrangements of lines of force and development of poles in certain directions in the case of rotating bodies, or otherwise, are the rule, and there is no correspondence whatever to the mode of action of any other force of which we have knowledge. Thus at no point throughout the research, as above outlined, has there appeared to be even the slightest "chance" for an alternative hypothesis. The evidence in favor of each proposition stated has been adequate and all in one direction, and moreover, taken together it is cumulative; each point strengthening the others and nowhere developing any inconsistencies. Professor Ashe is mistaken in stating that there has been "no attempted refutation." I have letters and articles by the score from persons who started in with vehemence, some of them many years ago, but who have gradually become very respectful, finally being brought to a realizing sense, that it is facts and not a personality against which they had been contending.

M. A. VEEDER.

Worms in the Brain of a Bird.

APRIL 7th, 1890, two common Bitterns (*Boturus mugitans*) were brought to me to be mounted. One of them was still alive but did not seem to be just natural, seemed to lack what we might call bird intelligence, and was smaller than the other and poor in flesh. This bird was given to one of my pupils in taxidermy, Miss Bernice Pike, to mount. When the head had been skinned and was ready to sever from the neck, which was done by cutting through the skull, the brain-cavity was found to contain a mass of thread-worms, occupying about one-third of the brain cavity. These were seemingly like the ordinary Gordius or Hair Snake, about the size of a Gordius that is three inches long, and coiled in a mass in the upper posterior part of the brain, and extending some down into the spinal canal. As near as I could say without removing them, they occupied the subarachnoid space,

and had absorbed much of the cerebrum and the upper part of the cerebellum, the outlines of the Arbor Vitae being plainly visible. I have never seen anything like this in any other bird, nor have I seen any account of worms being found parasitic in the brain before.

G. H. FRENCH.

Carbondale, Ill.

Epidemic Forms of Mental or Nervous Diseases or Disorders.

IN response to the inquiry concerning "epidemic forms of mental or nervous diseases or disorders," in the issue of May 19, I send the following account of an incident which "came to pass" under my own eyes. Several years ago our next-door neighbor's little girl, perhaps five or six years old, met with an accident which rendered it necessary that she use a crutch. Another little girl of about the same age, who lived in the adjoining house, seeing the little lame girl with her crutch, obtained a stick which she used as a crutch, hopping and limping, just as she saw the little lame girl doing. At first no attention was paid to this childish fancy, this imitation, this "playing being lame." After some days had elapsed, however, and this play became so constant as to be annoying, the stick was taken away, and the little girl told to put her foot to the floor. She screamed and cried and insisted most strenuously that she could not put her foot to the floor: she could not stand upon it, etc. I cannot say how long she persisted in thinking she was lame, but shall never forget how real her apparent affliction was to her, nor her screams of pain when she declared she could not stand without her "crutch."

MRS. W. A. KELLERMAN.

Columbus, O.

The Winter of 1719.

"IN the famous winter of 1709 thousands of families perished in their houses, the Arabic Sea was frozen over, and even the Mediterranean."

The above is found in a foot-note on page 39 of Professor Meech's article on the intensity of the heat and light of the sun at different latitudes, published in one of the "Smithsonian Contributions to Knowledge." It seems incredible. The "Arabic Sea," I take it, is what we call the Arabian Sea, or at least some one of the bodies of water which border on the Arabian peninsula. No information is given as to where the "thousands of families perished with cold in their houses."

The Arabian Sea measures about 500 miles across its narrowest part. Can it be possible that it was frozen over? or the Red Sea? or the Persian Gulf? or the Gulf of Oman? Has the Mediterranean Sea been frozen over either in historic, or even in glacial times?

The statement in Professor Meech's paper is made unequivocally, as if speaking of a well-known and well-established fact. It is not put in quotation marks, nor is any authority cited.

A few weeks ago I wrote to Professor Langley, secretary of the Smithsonian, asking for any information which he might be able to give me. To-day I received the following reply:—

"I beg to say that I do not know the original source from which his particular statements were derived, but that the winter in question was one of exceptional severity is fully attested by well-authenticated records.

"Under the article 'Temperatur,' in Gehler's 'Physikalisches Wörterbuch,' it is stated that at Paris the thermometer fell many times to -19° C.; that in the Kleiner Belt of the Danish seas the ice extended so far from the coast that the end of it could not be seen from the highest towers; and that the cold must have been especially intense in southern Germany and Italy, since the Adriatic Sea was wholly covered with ice.

"Additional records of this severe winter will probably be found in the memoirs of E. Brückner, who has been making an extended study of variations in climate."

There is nothing in the fact that at Paris the thermometer fell to -19° C., or $3\frac{1}{4}^{\circ}$ below our zero, nor that in the Danish waters ice formed far out from the coast to compare in any way with the freezing over of "the Arabic Sea or the Mediterranean!"

The fact—if it be a fact—that the Adriatic Sea was wholly covered with ice would indeed be very remarkable, but even that

was a small affair (indicating a moderate climatic aberration) in comparison with a temperature so abnormally low as to freeze over so large a body as the Mediterranean, or one so large and so exceptionally warm as the Arabic Sea.

Can any reader of *Science*, or any one else, throw any light on this subject?

C. B. WARRING.

Animal Effigies.

CAN you or any of your readers furnish me with a reference or references as to large numbers of small animal effigies of pottery found together in any mound of the United States?

C. B. M.

BOOK-REVIEWS.

The Moon's Face: A Study of the Origin of its Features. By G. K. GILBERT. 52 p. Washington, April, 1893.

THE present paper, although delivered as the presidential address before the Philosophical Society of Washington last December, has only recently been distributed in its complete form. Mr. Gilbert is well known as a geologist and a student of topographic form, and in this paper he has carried his studies away from things terrestrial and turned his eyes and his attention for a time to things celestial. The observations upon which the paper is based were made during three months of the past year, eighteen nights being available for the work, and the 26 $\frac{1}{4}$ -inch refractor of the U. S. Naval Observatory being the instrument employed. Numerous laboratory experiments were also carried on, and the literature relating to lunar topics was searched. The craters, as the most conspicuous features of the moon's face, are mainly dealt with in the paper, and after a description of their characteristics and a statement of the various theories advanced to account for them, the author advances a theory of his own. The volcanic theory is one held by many writers, but a comparison of terrestrial and lunar craters, even when the differences in condition are considered, led Mr. Gilbert to reject the hypothesis as untenable. The "bubble" theory, advocated by Robert Hooke in his *Micrographia*, in 1667, is mentioned, but as Mr. Gilbert had not seen the book the theory is not discussed in any detail. It may not be amiss to devote a few words to it here.

Hooke describes the features of the craters as he saw them through his telescope, and gives an illustration of some of them. Except as regards detail and the characteristic central hill shown in Mr. Gilbert's figures, those given by Hooke are very similar. In describing the craters he says: "These seem to me to have been the effects of some motions within the body of the moon, analogous to our earthquakes, by the eruption of which, as it has thrown up a brim or ridge round about higher than the ambient surface of the moon, so has it left a hole or depression in the middle, proportionally lower." He also mentions, what is of more interest, that he had made several experiments to ascertain, if possible, the origin of the pits. "The first was with a very soft and well-tempered mixture of tobacco-pipe clay and water, into which, if I let fall any heavy body, as a bullet, it would throw up a mixture round the place, which for a while would make a representation not unlike these of the moon, but considering the state and condition of the moon, there seems not any probability to imagine that it should proceed from any cause analogous to this; for it would be difficult to imagine whence those bodies should come; and next, how the substance of the moon should be so soft; but if a bubble be blown under the surface of it, and suffered to rise and break; or if a bullet or other body sunk in it be pulled out from it, these departing bodies leave an impression on the surface of the mixture exactly like those of the moon, save that these also quickly subside and vanish. But the second and most notable representation was what I observed in a pot of boiling alabaster, for then that powder being by the eruption of vapors reduced to a kind of fluid consistence, if, whilst it boils, it be gently removed beside the fire, the alabaster presently ceasing to boil, the whole surface, especially that where some of the last bubbles have risen, will appear all over covered with small pits exactly shaped like these of the moon, and by holding a lighted